west, what has been pointed out concerning the fading of the rest of the sky, the comparative localisation of light and colour in the west, and the illusion as to brightness that occurs when the background fades, the phenomenon of Alpenglühen, and that of the greenish illumination so often seen, can be accounted for without the help of the startling hypothesis quoted.

But it would be more satisfactory if observations could be made from above. Would M. Vallot sacrifice himself and spend some nights up in the observatories that he directs?

R.N.E. College, Devonport. W. Larden.

The Positions of Retinal Images.

The thanks of your psychological readers are due to Mrs. Ladd Franklin for having, in her letter published in your number of February 13, called attention to Schön's experiments, which, as she says, have been unaccountably overlooked. I have in consequence been repeating the experiment which Mrs. Franklin describes, but so far with purely negative results. Although some of the observers gave answers which might hastily have been interpreted as confirmations of Schön's illusion, a further analysis showed conclusively that no one on whom I have experimented, so far, perceived it.

Allow me to indicate one or two points in Mrs. Franklin's letter which seem to require elucidation. She writes as if the object looked at in the experiment "consists of a single bright point." But surely the point H in her diagram—the fixation point—was a bright point as well as O or O′? In Schön's experiments the apparent distance of O or O′ was judged relatively to H (which was a stick of phosphorus), by the "stereoscopic (or pseudoscopic) effect," and his explanation of the illusion was that we (unconsciously) judge as if the image actually produced on the right retina had been produced on the left, and *vice versa*. The image on each retina consists of two bright points, but cannot strictly be called a "double image," since the bright points are produced by two distinct objects—by the phosphorus at H, and by the light proceeding from O′ or O. If Schön's explanation is correct, then, supposing the light really proceeds from O′, and when the ray O′ *r′* is darkened appears to come from O, the observer ought to say the object appeared to be not merely as far off as H, but a *long way* behind it. Further, if the ray O′ *l′* is darkened instead of O′ *r′* there ought to be no illusion—he should say the object appears much nearer than H (*i.e.* still at O′); and if either ray is cut off altogether, he will have no reason for judging the object to be at O′, but will probably judge it to be further back—where the source of light actually is. In my experiments, so far, none of the observers have made any distinction between cases where the ray O′ *r′* was darkened and those where the ray O′ *l′* was; but if either of them was darkened considerably, they answered just as they did when one of them was totally extinguished; judging the object to be about where the source of light actually was—which was about the same distance as the phosphorus mark H, and very much nearer than the point O would have been.

I hope to continue the experiments, if possible until I get a positive result, and should be glad therefore to hear some further details of Mrs. Franklin's experiments, especially with reference to the points I have brought forward, either privately or through your columns. Edward T. Dixon.

4 Cranmer Road, Cambridge, April 17.

Colour Variations in Ducks and Pigeons.

About a year ago you published a short article by Mr. Francis Galton (April 11, 1895, vol. li. p. 570), in which he urged the desirability of making careful records of all cases of "sports" sudden variations in domesticated animals, &c. Two such sports having arisen recently under my own observation, one in ducks and one in pigeons, I write to place the facts before your readers.

(1) *Ducks.*—In January 1894, I bought in Beyrout market a drake of the common "Mallard" colours and four ducks, two of normal wild-duck colour, one pure white, and one black, splashed with white. From these ducks I raised, the same season, thirty-six ducklings; and, from eggs given by a friend, nine more. Concerning the latter, nothing need be said at present, except that their own mother was of a very dark, dingy brown, and the ducklings were nearly black in the down. Of the thirty-six ducklings hatched from my own ducks' eggs, twelve or thirteen (I neglected to note the exact number at the time) were different in colour from their olive-green brethren and from anything I had seen before, being of a beautiful pale fawn colour above, shading into canary-yellow beneath, with darker pencillings and shadings on the sides of the head and back, and with the normal, symmetrical series of three pairs of light marks on the upper surface, distributed just as in normal, olive-green ducklings. The entire set of these pale ducklings proved to be females, and their plumage, when adult, was a pretty yellowish or sandy buff colour, with darker shadings, due to a brown streak down the middle of each of the contour-feathers. The speculum on the wing gave mostly sky-blue reflections, instead of the usual metallic green of common ducks. Two only of the dozen (or thirteen) differed perceptibly from the others, being of *uniform* cinnamon-brown colour, with white throats.

Five of these pale ducks were kept and allowed to breed, viz. one cinnamon-brown and four yellow ones. In addition, my stock during the season of 1895 consisted of three of the original old ducks (one white, one black, and one normal); three normal-coloured young ducks related to the pale ones (*i.e.* same paternity, and presumably same maternity to some extent); and two ducks raised from the eggs given by my friend, as above mentioned, and therefore non-related to the others—in all thirteen ducks. Of drakes there were four—two of normal mallard colour (related, as above, to the pale ducks), and two own brothers to the dark ducks, these having green heads and beautifully-pencilled stone-grey bodies, with no brown on the breast and no white collar—a departure from typical drake-colouration which is normal (in Syria at least) to dark varieties.

From this stock of ducks I raised last spring sixty-two ducklings, of which nineteen were fawn-coloured in the down. One of these died very young. Of the remainder, fourteen were females and four males. All were sandy-buff, none cinnamon-brown; but one—a female—was a shade or two darker than the rest, and when adult showed no metallic colours on the speculum, agreeing in this respect with the dark ducks of alien parentage.

Of greatest interest to me was the question: What will the "yellow" drakes be like when adult? Time has answered as follows: Head and neck, soft *coffee brown*, with obscure greenish reflections in some lights; narrow white collar; chestnut-brown breast, similar to mallard; upper tail-coverts (including curled feathers), and under-tail coverts, chocolate-brown; the rest delicate cream colour, with fine transverse pencillings on back and sides, similar to those on the mallard, but paler and less distinct: the whole effect very pleasing.

Of course all this may be familiar enough to some people, but it is quite new to me, and no mention of such drakes is made by Darwin in "Animals and Plants," nor by any other writer whose works I have been able to consult. Whether atavism has anything to do with the matter, I cannot say, as the parentage of my original stock is entirely unknown; but I am accustomed to notice very carefully all the ducks I see about town and the surrounding country, and am sure I have never come across any such during an experience of about twenty-five years. In any case, it is interesting to note that the new variety was far from being "swamped" by the inevitable crossing with its parent form.

(2) *Pigeons.*—In 1894 I procured a pair of birds of a variety known to Arab fanciers as black *Urjani* (or *Shamandarûzi*). These are largish pigeons, wholly black, with two "red" (*i.e.* bright reddish brown) bars on each wing, corresponding to the black bars on normal "blue" pigeons. The pair were unrelated, the male coming from Hums, the female from Damascus. The variety is scarce in Beyrout, and is valued more or less by all Syrian fanciers, who breed it with some care; and it habitually breeds true. My birds produced during the season of 1895 ten young ones: six (3 ♂, 3 ♀) quite normal in colour; one (♀) slightly mottled on the shoulders with brown and a very little white; and three (all ♀), which in the nest plumage were uniform *light red*. (I had not a red bird in the loft—scarcely a red feather, aside from the red bars of the *Urjanis* themselves, so there was no question of illegitimate paternity.) But, strange to relate, when these red birds moulted, nine-tenths or more of their red feathers were replaced by *pure white*, so that their adult plumage may be described thus: *white* birds with red neck, abdomen red mottled with white, a very few red feathers scattered over back and shoulders; no trace of red bars.

Careful inquiry among Arab fanciers having personal experience of the breed in question, elicited the following

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information. Black *Urjanis* usually breed true; when they fail to do so, the progeny is generally a sport of a particular kind called *dijji*, uniformly red when young, more or less mottled with white when adult. These *dijjis* are apt to "throw back," and in turn produce good *Urjanis*.

One of my own mottled birds (*dijji*) remains in my possession, and is now mated to an *Urjani*—an own brother. The pair has produced this spring four young: three *dijjis*, just like the mother, and one partial reversion to the *Urjani* form, being dark-chequered blue, with red bars on the wings. The original parent pair of *Urjanis* have also raised four squabs this season—three normal and one *dijji;* sexes not yet determined. I state these facts without comment; but would be glad to know whether fanciers in England or elsewhere have observed anything quite as striking in the way of colour-variation.

Beyrout, Syria. W. T. VAN DYCK.

Dependence of the Colour of Solutions on the Nature of the Solvent.

IT is a well-known fact that the colour exhibited by one and the same body in solution depends more or less on the nature of the solvent. In some cases this phenomenon can be satisfactorily accounted for by electrolytic dissociation, but in the majority of cases hitherto examined this explanation is not admissible. Perhaps the most striking of these is that of iodine, the solutions of which are coloured variously violet, blue, brown, and yellow. The hypothesis has been put forward that the variation in absorption might be due to the formation of molecular aggregates of variable complexity; but this, at least in the case of iodine, has been rendered very improbable by the recent researches of Beckmann and others. Nor does the hypothesis that the variation may be due to a varying degree of combination with the solvent seem much more promising.

If, now, absorption be a case of electrical resonance, should one not expect a relation between the absorption of the dissolved body and the physical properties of the solvent, sufficient to account for the observed variations? That such a relation should exist, seems possible from the following rough considerations.

The period of vibration of an electric oscillator is, in the usual notation,

$$T = 2\pi\sqrt{LC},$$

where L = self-induction, and C = capacity. But now:—

$$LC = gK\mu,$$

where g is a geometrical factor and K and μ are the dielectric constant and permeability of the surrounding medium. Also $n^2 = K\mu$, where n is the index of refraction of the medium for very long waves, whence it follows that

$$T = 2\pi n\sqrt{g},$$

which means that the principal absorption-band should travel towards the red end of the spectrum as the index of refraction of the solvent increases. This result is identical with the general qualitative law enunciated many years ago by Kundt, on the basis of experimental data. There are, it is true, various breaks in the parallelism; still this mode of viewing the question seems to offer more possibilities than the others.

Holywood, Belfast. F. G. DONNAN.

Hatching Lizards' Eggs.

CAN any of your readers suggest a way to hatch lizards' eggs? I have had a pair of bright-green lizards (I think they came from Italy) in a glass vivarium in a very sunny window for two years and a half. Last year, on May 19, the female laid eleven eggs. I left them exactly as the mother laid them, and after about three weeks I opened one and found the rudiments of a young lizard; but the other eggs never came to anything. I should like to rear them this year if it is possible. H. A. ROSS.

Trevean, Penzance.

THE DIFFUSION OF METALS.

IT is now quite usual to think of alloys as being solid solutions and to recognise that the atoms of solid metals are in active movement. That this must be the case, is revealed by the passage of metals to allotropic modifications in which the physical properties differ widely from those of the same metals in their normal state. It is well, therefore, that we should remember how much was done for us thirty years ago by Matthiessen in framing such views, and by Graham in showing that solid metals are true solvents for gases which move and diffuse freely in them, sometimes to reappear with gaseous elasticity.

The experimental portion of the latter work, Graham entrusted to me, and my hope that I should be able to extend his work on the diffusion of salts, to liquid and solid metals, has been somewhat tardily realised by the delivery in the present year of the "Bakerian Lecture" of the Royal Society, of which the following is a brief abstract.

PART I.—*Diffusion of Molten Metals.*

In the first part of it allusion is made to some earlier experiments of my own conducted in 1883 on the diffusion of gold, silver, and platinum in molten lead. It is strange that although the action of osmotic pressure in lowering the freezing point of metals has been carefully examined, very little attention has been devoted to the measurement, or even to the consideration, of the molecular movements which enable two or more metals to form a truly homogeneous fluid mass. The absence of direct experiments on the diffusion of molten metals is probably explained by the want of a sufficiently accurate method. Ostwald has stated, moreover, with reference to the diffusion of salts, that "to make accurate experiments in diffusion is one of the most difficult problems in practical physics," and the difficulties are obviously increased when molten metals diffusing into each other take the place of salts diffusing into water.

The continuation of the research was mainly due to the interest Lord Kelvin had always taken in the experiments. The want of a ready method for the measurement of comparatively high temperatures, which led to the abandonment of the earlier work, was overcome when the recording pyrometer was devised, and the use of thermo-junctions in connection with this instrument rendered it possible to measure and record the temperature at which diffusion occurred. Thermo-junctions were placed in three or more positions in either a bath of fluid metal or an oven carefully kept hotter at the top than at the bottom. In the bath or oven, tubes filled with lead were placed, and in this lead, gold, or a rich alloy of gold, or of the metal under examination, was allowed to diffuse upwards against gravity. The amount of metal diffusing in a given time was ascertained by allowing the lead in the tubes to solidify; the solid metal was then cut into sections, and the amount of metal in the respective sections determined by analysis.

The movement in linear diffusion is expressed, in accordance with Fick's law, by the differential equation

$$\frac{dv}{dt} = k\frac{d^2v}{dx^2}.$$

In this equation x represents distance in the direction in which diffusion takes place, v is the degree of concentration of the diffusing metal, and t is the time; k is the diffusion constant, that is, the number which expresses the quantity of the metal in grams diffusing through unit area (1 sq. cm.) in unit time (one day) when unit difference of concentration (in grams per c.c.) is maintained between the two sides of a layer 1 cm. thick. The experiments described in the Bakerian Lecture showed that metals diffuse in one another just as salts do in water, and the results were ultimately calculated by the aid of tables prepared by Stefan for the calculation of Graham's experiments on the diffusion of salts, special tables being calculated by one of my students, Mr. A. Stansfield, in connection with this research.

The necessary precautions to be observed and the corrections to be made were described at length and the

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